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ESP TESTS WITH MENTAL PATIENTS
BEFORE AND AFTER ELECTROSHOCK
TREATMENT

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THE hope of finding some particular physical or mental state of personality associated with highly successful psi functioning has led to a number of studies of ESP in various types of mental patient in whom extremes of personality expression are present. In the late 1930's several researches were carried out in mental hospitals, but the results were not clear cut: Shulman's only significant result came from manic-depressive depressed patients who scored quite positively (3); the reported data of Van Wiemokly's unfinished experiment appear to be without significance (3); and Margaret Price obtained generally significant positive results from various types of mental patients with no significant differences in ESP scores for the different psychiatric classifications (1).

Little more was done with mental patients as ESP subjects until the late 1940's when a new impetus to this type of research was imparted by the work of Professor Dr Hubert J. Urban and Mr Friedrich Köck in Innsbruck, Austria (2). Under the direction of Professor Urban, Mr Köck gave ESP tests to patients before and after they underwent various types of shock therapy. The early exploratory experiments were of the undifferentiated ESP type and were given to patients with various types of mental disorder. These tests produced very striking differences in ESP scores before and after shock treatment, with the patients scoring much higher after shock than before. Feeling some dissatisfaction with the loose exploratory character of the ESP test

conditions, the experimenters started a new series of tests using the more easily controlled clairvoyance procedure.

In midsummer 1950 I was able to visit Innsbruck where Mr Köck kindly showed me how he conducted the tests and discussed the various trends his data were showing. These results interested my colleague, Miss Elizabeth McMahan, and myself so much that when we arrived in London in mid-August to do research under S.P.R. auspices,¹ we resolved to take advantage of the excellent mental hospital facilities in London. We carried out a series of 'clairvoyance' card tests with mental patients just before and just after electroshock treatments. Later in the series we were able to try a few general ESP tests (hereafter called 'GESP' tests) with some of the same patients.

The primary ESP technique used was a modified 'BT' procedure; that is, one experimenter held a pack of ESP cards behind a screen and, without looking at the cards, removed them one by one from the pack as the subject called each card by pointing to one of five sample cards. The subjects were asked to point out their choices because some patients, especially just after shock, were unable to recall the names of the symbols. The second experimenter recorded the subject's choices. The cards used in the tests were prepared beforehand by several S.P.R. members who gave unstintingly of their time for this tedious task.² Using random number tables (4), they made up the packs of cards and attached to each a record of its order while retaining a duplicate copy of all card orders. Thus, in a test session, we were able to check the subject's calls immediately without having to pause to record the pack.

In approximately three-fourths of the series, Miss McMahan was the experimenter who recorded the subject's calls while I handled the cards behind the small upright screen. In the latter part of the series Mr G. W. Fisk was the experimenter holding the cards while I recorded for the subjects.³ In these later tests we were often unable to obtain new subjects not previously tested, and it was then that we tried GESP tests on subjects who had had clairvoyance tests earlier.

¹ It is a pleasure to acknowledge my indebtedness to the Society for Psychical Research for the research grant which made possible a visit to London to carry out this experimental work as well as other research studies.

² Special thanks are due to Mr G. W. Fisk who bore the brunt of the work of preparing the cards for this research. Dr D. J. West, Miss Pamela Clark, and Mr W. E. Manning also kindly aided in this task.

³ When Mr Fisk was the experimenter, we used packs not prepared by him.

The experiments were carried out at Springfield Mental Hospital and at Northumberland House (a private mental institution). The staffs of both institutions were most helpful and went to a great deal of trouble to provide us with patients and with comfortable working quarters. With only a few exceptions the tests were carried out in private rooms.

The usual procedure was as follows : on the mornings on which shock treatments were to be administered in the hospitals, we tested each patient individually by giving him four runs with the ESP cards. As soon as possible after the shock, we retested each in another four runs.¹ The time elapsing between the shock and the second ESP test was by no means uniform. Some patients were able to respond in the ESP test within 20 minutes after shock. Others were still very much dazed and disoriented 40 to 60 minutes later. Many patients were much slower in making their responses after shock, and the delay with these patients meant a longer lapse of time before we could proceed to others. We kept full notes on the time intervals, but later analysis showed no differences in test results.

At both hospitals the patients receiving electroshock therapy were primarily those persons classified as 'depressed'. Occasionally their records indicated involutional melancholia or the manic-depressive depressed condition, but in reporting the results I shall adopt the classification 'depressed' as the most uniform and general term for this group. A few schizophrenic patients and several with miscellaneous disorders were also tested.

The patients were tested at various stages in their series of shock treatments. We were not able to confine our tests to persons receiving their first shock. We therefore made it a practice to test as many patients as possible just before and just after a given shock treatment. From the hospital records we obtained the information as to how many previous treatments each patient had had. Some had had previous courses of electroshock or insulin shock therapy or even lobotomy operations, and their case histories differed widely.

RESULTS

In all, 56 test sessions were held ; in 28 of these, the patients were given clairvoyance tests before and after electroshock. The

¹ In both hospitals the amount of shock was regulated to try to produce a *grand mal* seizure. In a few cases only a minor convulsion resulted. From these few cases it is impossible to determine whether the amount of shock or the type of reaction was in any way associated with changes in ESP scoring levels.

results of these clairvoyance tests are shown in Table 1. None of the figures attain statistical significance. The most interesting figure is perhaps that for the few schizophrenic patients before shock when they made 16 runs with a deviation of +20 ($\chi^2 = 6.25$; $P = .012$).

TABLE 1

RESULTS OF CLAIRVOYANCE TESTS GIVEN BEFORE AND AFTER ELECTROSHOCK

Diagnosis	No. Cases	PRE-SHOCK		POST-SHOCK	
		Runs	Dev.	Runs	Dev.
Depression					
Men	11	44	- 6	44	+ 9
Women	12	48	+ 6	48	- 13
Total	23	92	0	92	- 4
Schizophrenia	4	16	+20	16	+2
Anxiety Neurosis	1	4	- 2	4	+ 1
Grand Total	28	112	+18	112	- 1

The depressed patients constitute the largest group and the results of the men and women are shown separately. A breakdown of these cases shows no features of interest; ten patients were better in their pre-shock ESP tests, twelve scored higher in their post-shock tests, and one gave the same score on pre- and post-shock runs.

One matter of passing interest might be recorded: the women patients in the depressed group showed a tendency to (+1) forward displacement. Before shock, their trials gave the equivalent of 46.08 runs with a deviation of +29.6 ($\chi^2 = 4.75$; $P = .029$); after shock they gave 45.12 runs¹ with +14.40 on (+1) displacement. Thus the total (+1) displacement score for these women patients was positive (+44 in 91.2 runs) and gives a chi square of 5.31 which is of some interest although a selected case ($P = .021$). No such tendency to displace forward was shown in the data of the male depressed patients.

In the latter part of the series, when we had difficulty in securing new subjects, we tried GESP tests with eight patients who had already taken clairvoyance tests. The procedure was the same except that here the experimenter looked at each card behind the screen. (In these tests the agent was not permitted to speak to the subject during the course of a run.)

¹ Differences in number of pre- and post-shock trials are due to the fact that a few subjects were unable to complete their post-shock runs. Because the discrepancy was so small, the figures for the direct hits in Table 1 were not corrected for the loss of these few trials in the data of some subjects.

The results of the GESP tests were :

Diagnosis	No. Cases	PRE-SHOCK		POST-SHOCK	
		Runs	Dev.	Runs	Dev.
Depressed	6	24	- 11	22*	+ 12
Schizophrenics	2	8	+ 8	8	- 4
Total	8	32	- 3	30	+ 8

* This figure is two runs less than the pre-shock runs because one subject was physically unable to finish her post-shock tests.

The data are too few to permit conclusions. Five of the subjects (all depressed) were better after shock than before ; one depressed patient and one schizophrenic scored the same on both tests, while the other schizophrenic did better before shock.

In the course of the series, ESP tests were given to 11 other patients, some participating in several sessions. For the most part these subjects were patients who were scheduled to receive shock but some change in plan resulted in their not taking it. A few were patients whom the attendants asked us to test because of some particular feature of interest in their case histories. Two were persons who were considered recovered and were actually released a few days later. The results of all these tests (clairvoyance) were as follows :

Diagnosis	No. Subjects	No. Sessions	Runs	Dev.
Depression	8	16	64	+ 18
Schizophrenia	1	2	8	+ 10
Miscellaneous	2	2	8	+ 6
Total	11	20	80	+ 34

$$\chi^2 = 3.61$$

$$P = .06$$

These results were all positive and the deviation was approaching the point of statistical importance, but no conclusions can be drawn.

COMMENTS

The results of this research were in general disappointing. The depressed patients who made up the bulk of the subject group showed very little of interest in either pre- or post-shock ESP tests. The earlier work of Shulman (3) suggested that positive results might be expected from manic-depressive depressed subjects. Those subjects in our London group who were specifically labelled as manic-depressive depressed gave no higher

results than the others and their results were close to the chance level. No differences attributable to age or to the number of previous shocks were found. Neither were there any significant position effects.

One point of possible importance for further research emerges from these data—that is the consistently positive scores obtained from the few schizophrenic patients available. If we summarize all their results we get the following figures :

<i>Type of ESP Test</i>	<i>No. Sessions</i>	PRE-SHOCK		POST-SHOCK		TOTAL	
		<i>Runs</i>	<i>Dev.</i>	<i>Runs</i>	<i>Dev.</i>	<i>Runs</i>	<i>Dev.</i>
Clairvoyance	4	16	+20	16	+2	32	+22
GESP	2	8	+8	8	-4	16	+4
Clairvoyance (no shock given)	2	8	+10			8	+10
Total	8	32	+38	24	-2	56	+36

$$\chi^2 = 11.29$$

$$P < .001$$

$$\chi^2 = 5.81$$

$$P = .017$$

Taken as a group their results are significantly positive. The tests done in the 'normal' state—that is, without shock effect—are highly significant with a probability of less than one in a thousand. The significantly high scores of these patients in the normal state poses a problem: are these results due to some combination of circumstances peculiar to these specific subjects or do they suggest that high scoring may be expected generally from schizophrenic patients? In general, previous research with schizophrenic subjects has not produced such consistently high scores. Dr D. J. West (in research which followed that reported here) tested a group of psychotic patients and did not notice any tendency to high scoring on the part of the schizophrenics (5). From his report it seems clear that his subjects were more disturbed and less amenable than ours.

Perhaps some light may be thrown on the problem if a fuller description of this particular group of patients is recorded. The four patients who took clairvoyance tests before and after shock all scored positively in the pre-shock tests, their average run scores being 5.75, 6.00, 6.50, and 6.75. After the shock two of them scored positively and the other two scored slightly below chance.

Two of these four also took GESP tests before and after shock. Both were positive before shock (averages of 5.50 and 6.50); after shock one scored positively and the other scored well below chance. Thus, in before-shock tests, both clairvoyance and GESP, *all* schizophrenic patients were positive.

Three of these four patients were schizophrenics of long standing (eight or nine years). (For the fourth patient this information was not available; we know only that the present episode had begun four or five months earlier.) Two of the four were also diagnosed as mentally deficient; a third was classified as 'dull in intelligence'. Two case histories mention that the patients suffered delusions of persecution by telepathy.

In the ESP test sessions all four subjects were quite co-operative and easy to handle (in contrast to the depressed patients tested). At this particular period, the psychiatrists had noted no improvement from the shocks in three of the four cases (no information given for one patient). And yet in our contact with them, they showed no bizarreness or distortion of personality such as is sometimes encountered in schizophrenics. In spite of three of them being diagnosed as low in intelligence, they all seemed to understand the tests readily. All of them made their calls rapidly (approximately one call every two or three seconds), both before and after the shock.

One other schizophrenic patient was given clairvoyance tests in two sessions without shock. He was a highly intelligent man, quite distinguished in his own field. Previously, he had been extremely violent and had been given a course of insulin-shock treatments. At the time we tested him for ESP, he showed a marked improvement and shortly after our second session with him, he was considered recovered and was released from the hospital. In the eight runs he did in all, only two were slightly below the chance level, the remaining six scores ranging from 6 to 10. Throughout the tests, he disclaimed any ESP ability and insisted that such card-guessing could only yield chance results.

The outstanding characteristic of this group of patients was their co-operativeness in the ESP tests. They were all pleasant, responsive, and easy to work with. Intellectual level does not seem to have been of importance: three were classified as low in intelligence, but one was highly intelligent (no information on one patient). None of them showed the marked deterioration of personality sometimes seen in schizophrenics.

These details concerning our group of high-scoring schizophrenics may enable psychiatrists to judge whether or not these patients present a somewhat unusual clinical picture. It would appear from the earlier studies that high ESP scoring cannot be expected from schizophrenics in general. But schizophrenia is a complex classification; it has been described as 'the mystery of psychiatry'. The disorder may take a number of different forms;

and patients may pass through different stages on their way to recovery or to deterioration. Is it possible that our schizophrenics represent a certain stage in the course of this mental illness where psi functioning might be facilitated? It would be of some interest to know whether psychiatrists can help in this problem of interpreting the highly successful ESP scores of this small group of patients. Admittedly their numbers are small, but their ESP scores are high enough and consistent enough to suggest that this is a lead worth pursuing.

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ANTOINE RICHARD'S GARDEN

A POSTSCRIPT TO 'AN ADVENTURE'

(continued)

BY G. W. LAMBERT, C.B.

PART III

IN *Apparitions* G. N. M. Tyrrell remarked that there is no independent evidence to show that subjective hallucinations are ever collective.¹ There is thus strong ground for persevering with the attempt to discover the significance of the remarkable and detailed visions seen by Miss Moberly and Miss Jourdain, on the assumption that they were not merely unintelligible dream-stuff. At the same time, one must not underestimate the difficulties of verifying a hypothesis like that put forward in my paper in the *Journal* for July-October 1953. On one side of the comparison there are the descriptions by the two observers of things seen 'life size', and on the other there are markings on a small plan, which have got

¹New ed., London, Duckworth, 1953, p. 149. See also p. 114.

to be imagined 'life size' in elevation, a process in which one may easily make a mistake.

To the descriptions in A, helped out by Miss Moberly's sketch of the kiosk (Fig. 4)¹ we cannot now add anything. To Richard's plan (Fig. 3) a good deal can now be added by way of elucidation, as the result of further historical research. In a study of this kind, if one is on a completely false trail, one is usually confronted at a fairly early stage with a fact which flatly contradicts the working hypothesis. So far from encountering such a fact, I have found pieces of evidence, collected from various sources, which, while they do not afford incontestable proof, are cumulative in their effect. In short, they buttress the hypothesis, and do not undermine it.

I. THE KIOSK

M. Léon Rey has greatly simplified the issue here, by the statement in his letter, published in the *Journal* for January-February 1954 (pp. 249-51), that to the best of his knowledge no buildings in the Chinese style, other than the Jeu de Bague, were ever actually erected in the Garden. On the assumption that there was genuine retrocognition at work, we must either suppose that the kiosk seen by the two observers was a distorted image of the central part of the Jeu de Bague, as M. Rey suggests, or else that it was a purely imaginary design, which somehow got substituted for Richard's original design. I preferred the second alternative owing to the remarkable similarity between the spectral kiosk and Sir William Chambers's No. 1 design as shown in Fig. 2.

The suggestion that Chambers had some influence on designs intended for the garden of the Petit Trianon receives considerable support from the following facts :

(1) In 1765 Antoine Richard went to England on a botanical journey which extended to Scotland and Ireland, and included a visit to Kew Gardens, where he saw Chambers's new buildings. It must be remembered, however, that Richard was a botanist, whose interest was in trees and plants. He would not, at that time, have taken much interest in architectural garden ornaments, as they were outside his province.²

¹ Figures 1 to 4 appear in Part II of 'Antoine Richard's Garden' (*Jnl. S.P.R.*, 1953, xxxvii, 123-54).

² For Richard's journey see J. A. Le Roi, *Coup d'oeil retrospectif sur quelques faits historiques de 'Horticulture Versailles'* (Paris, 1865), p. 39 ; G. A. Desjardins, *Le Petit Trianon* (Paris, 1885), p. 24 ; and P. de Nolhac, trs. F. M. Robinson, *The Trianon of Marie-Antoinette* (Fisher Unwin, 1925), p. 83.

(2) Chambers's unpublished Letter Books¹ throw a good deal of light on his visit to Paris in 1774. He arrived there during the first week of May, just before Louis XV died. During his stay he was ignored by the British Embassy, but was very hospitably entertained by a number of French friends, notably by two fellow architects, J. D. Le Roi (1728-1803), whom he had known for many years and had used as his selling agent for his books in Paris, and C. de Wailly (1720-98), 'architecte du roi,' who entertained Chambers both in Paris and at Versailles. It looks as if Chambers stayed at Versailles for part of his visit, as he complains in a letter of 30 May that he had come up specially from Versailles to see his correspondent who had failed to keep the appointment.² We may be certain that while there Chambers went to see the Petit Trianon, which had been built since he was last in Paris twenty years before. On 24 May 1774 Louis XVI gave the Petit Trianon to Marie Antoinette.³ This is consistent with the King's own statement in a letter dated 4 June, 'Last week I gave the chateau of the Petit Trianon to the Queen.'⁴ She had for some days past been letting some of her confidants know that she would like to have it, and Chambers may well have heard before he left Paris that it was hers, and that she wanted to make an English garden there. Early on the morning of 4 June, just as he was preparing to leave for England, Chambers received an urgent summons to go to see the Duc de Chartres, who wished to thank him and to consult him.⁵ We do not learn whether the meeting took place, or what was to be discussed. At the time the Duc had great influence with Marie Antoinette. It was he who put into her head the idea of making a Jeu de Bague in her garden, like the one he had at Monceau.⁶ I can find no evidence that Chambers ever had anything to do with a Jeu de Bague, but the facts I have cited make it practically certain that Chambers's designs for Chinese garden pavilions were being handed round at Versailles during the following few weeks, while the plans for the English garden were being discussed.

Incidentally, on p. 129 of my earlier paper I attributed the Richard design to young Claude, Antoine's son, because it was

¹ Sir William Chambers's Letter Books (British Museum, Additional MSS. 41135). References are to Vol. III.

² Fol. 26(b), 27(b), and 42.

³ Desjardins, loc. cit. p. 56, footnote 3.

⁴ *Letters of Louis XVI* (Paris, 1864), Tom. I, p. 33.

⁵ Chambers, Fol. 27(b).

⁶ In a list, dated 26 February 1776, of garden items not yet constructed appears 'Un jeu de bague, d'après l'idée de M. le Duc de Chartres ...' (A. N. No. O¹ 1875).

signed 'Richard fils'. That was a mistake. Old Claude Richard, Antoine's father, was still alive (he died in 1782), and in 1774 Antoine was still signing himself 'Richard fils', as will be seen from his declaration quoted below (p. 273). This further evidence leaves the mystery of the kiosk unsolved, but it strongly suggests that the solution is hidden somewhere in the events of 1774, and not in the normal experience of the two persons who saw it in 1901.

2. THE 'SECOND BUILDING'

I assumed (pp. 130 and 136) that the second 'house' or 'building' at right angles to the terrace, at the south-west corner, was identical with the small rectangular roofed building shown on Richard's plan as adjoining the terrace, and connected to the kitchen block by two covered ways. The plan of the Garden in Louis XV's time shows that small building to have been the 'réchauffoir', or reheating chamber, for keeping dishes hot on their long journey from the kitchens to the dining-room. It may have had a flat roof, but it is not clear how the chimney was placed. No picture is available, showing what this building and its surroundings looked like from the terrace. One can only guess that it was kept out of sight as much as possible.

In the light of further research, I now think that the 'second house' was the kitchen block, including the chapel, for the following reason. The arrangements at the end of that block, where it was connected to the House, as they existed before the chapel was built in 1773, are shown in a plan of the lay-out of the French Garden, prepared by the architect Gabriel, of which the original is in the Archives Nationales (O¹ 1885). (Part of it is reproduced in line and colour as Plate XXXII in *Ange-Jacques Gabriel* by the Comte de Fels.¹) It shows that at the corner of the kitchen block nearest to the House there were three ways out from the kitchens :

(1) on the ground floor level, into the passage leading to the House ;

(2) to the right, up a flight of 22 steps (a vertical rise of about 12 feet), to the first-floor level, and then forward through a stepped door onto a flat terrace, level with the top of the passage to the House ;

(3) to the left, through a door at ground floor level in the north wall, to a flight of some 13 steps leading up into the garden, which was some feet lower than the terrace at that point.

When the chapel was built in 1773, the kitchen passage seems to have been diverted round the end of it as shown in Mique's plan of

¹ Paris, 1912.

the offices in 1781 (Desjardins, pl. xii), and the choice of ways to the three points mentioned above was preserved. It also appears from Fig. 3 that a separate covered way to the chapel was made, parallel with the kitchen passage, and not on top of it. One may therefore infer, with some confidence, that the access to the terrace along the top of the kitchen passage, next to the 'cour', remained as it was before the chapel was built. Thus, in 1774 a person standing on the terrace outside the antichambre (where the two observers stood in 1901) could have seen a footman come out of a service door close to the chapel, and approach on the level, without going out of sight.

This suggestion not only accords with the detail of Gabriel's plan, but also explains why the two observers, if they saw the conditions then existing, afterwards thought they saw the man come out of the chapel. (A, pp. 94-7). The point is of some interest because they were informed that in Marie Antoinette's time the footway over the kitchen passage was covered, so that she could go to the chapel under shelter in wet weather. The Queen did a good deal to the kitchen block in 1780, and if the information given was correct, it would mean that what the ladies saw was consistent with the state of affairs in 1774, but not with that in 1789. It is possible, however, that the information was based on a misunderstanding as to the relative positions of the two passages. Anyhow, the two ladies went away with the impression that there were three ways, one over the other: at the bottom, the kitchen passage, over that the covered way to the chapel, which Marie Antoinette used, and on top of that the open air way along which the man came. That was an impossible arrangement, as there was not room for it in the vertical direction.

Incidentally, both observers remembered vividly that the door shut with a bang behind the man when he came out. A service door, to be kept shut when not in use, would probably have been closed by a cord and counterweight.

There is another interesting feature in Gabriel's plan. It shows that originally the kitchen block extended westwards from the House only to a distance about level with the circular pond, some half way to the Pavillon Français. The cross avenue at that point in the plan runs south at its full width with trees on either side, into the broad avenue leading to the front entrance. The two observers were aware of the former existence of such a road (A.p. 98), and claimed that it corresponded with their recollection of 'the roadway through which we passed in 1901' (ibid. p. 99). Richard's plan shows the service block extending without a

break some way further to the west, but Mique's plan of 1783,¹ and de la Motte's plan of the same year² both show the way out shown in Gabriel's plan as still existing. It is therefore difficult to judge what the position was in the summer of 1774. Perhaps when the building was extended to the west, an arch was constructed over the way out. In their appreciation of time and distance, the two observers do not seem, while on their walk, to have agreed very well either with one another, or with their actual movements. In the condition in which they were, it is by no means impossible that when leaving the garden in fact near the entrance to the lane, they *thought* they were going out by the broad way shown on Gabriel's plan much nearer to the House, since that is what they saw.

3. A REVIEW OF SOME DETAILED EFFECTS

At this stage, with such curious coincidences to account for, it seems worthwhile to go back to the beginning of their walk on 10 August 1901, and to review some minor 'incidents' to which I have given little or no attention because of their doubtful significance. As the two observers took the trouble to mention them, they must have been doubtful as to their reality.

1. *The old-fashioned plough* (A, pp. 53 and 68). If this object had any significance at all, it was surely 'symbolic' of Louis XV, who made use of it, rather than of Louis XVI, who merely left it lying about.

2. *The livery worn by the two 'officials'*, met near the gardener's gate. As seen by the two observers, they wore tricorne hats, long greenish or greyish-green coats,³ and carried what Miss Jourdain thought were long staves⁴. In the very interesting picture of the *Jeu de Bague* which M. Léon Rey reproduced with his article in the *Revue de Paris* (December 1952), the man in the foreground behind the two ladies is depicted as wearing a tricorne hat and carrying a long staff, but no sword. In Desjardins' *Le Petit Trianon* (facing p. 1) is a picture of about 1782. In it the gentlemen wear wigs (no hats), carry swords, and in the right hand have sticks of ordinary walking-stick length. I therefore infer that the man in the former picture was a garden official, not a courtier. I had hoped to discover the colour of his coat from the original water-colour picture from which M. Rey obtained his illustration, but unfortunately the collection of the late M. Parmentier to which it belonged has been dispersed, and the water-colour cannot now be traced. M. Rey, however, has cited to me some evidence of

¹ A, Appendix.

² Desjardins, pl. xi.

³ A, pp. 45 and 53. The colours are given in the original narratives in A., 2nd. Edn.

⁴ A, p. 69.

even closer date. A record of 1775-6 shows that the gardeners (Richard father and son) wore green (A. N. No. O¹ 284/468). So far as one can judge, therefore, the appearance of the two officials seen by the two observers in 1901 corresponds very closely with that of the Trianon gardeners of 1774.

3. *The florid young man*, who came to the rescue near the kiosk (A, pp. 46, 54, and 76). His footsteps were heard before he could be seen, and he appeared suddenly 'either over, round or through the rock (or whatever it was)'. In Richard's Plan (Fig. 3), between the gardener's gate and the kiosk, the reader will find a curious feature, not numbered and described, which consists of five objects in a widening of the path, arranged as a quincunx. They are irregular in shape, and do not seem to be flower beds. The outer four may represent mounds about the height of a man, and the central object, which is nearly round, may represent a pillar-like rock, the object of the feature being to provide a sudden and surprising change of view for a person walking through it in either direction. I have not found a contemporary verbal description of such a device, but in two garden plans in Le Rouge's *Collection des Jardins Anglo-chinois* (1776) there are features having apparently the same object. In one case (Cahier 3, Plate 5) the feature is in a 'composition', i.e. a specimen plan. It has five 'elements', arranged in the same way as in Richard's feature. The outer four are clearly shrubberies, perhaps on mounds. The nature of the central element is not clear. The second case (Cahier 1, Plate 23) occurs in a plan for an English Garden designed by the Duc de Croÿ, the friend of Antoine Richard. It is placed in the wild garden ('futaye sauvage'). It contains two 'elements' only, arranged so as to give the middle pathway a diagonal slant. It is difficult to think of any reason for these features except to provide a sudden change of view. In the wild part of the Trianon garden, where Richard envisaged it, a feature of the same kind would not have been out of place. If one imagines the two observers standing just beyond it, and looking irresolutely across at the kiosk, they would have heard a man running towards them along the path from the gardener's house before they could see him. On turning round to see who was coming, they would have seen the man appear suddenly round the rock (or whatever it was). One must assume, of course, that this feature, like the kiosk, never existed outside Richard's imagination.

4. ANTOINE RICHARD'S CRISIS

There is a widespread belief that experiences of the kind here in question occur on or about the anniversaries of the events to

which they relate. One must allow some latitude, as the right persons are not always in the right place at the exact anniversary date. The ladies themselves disowned a belief in the special virtue of anniversaries, but that did not prevent Miss Moberly from seeing significance in the facts that their experience occurred on 10 August 1901, and that 10 August 1792 was a critical date in the history of the French Revolution. That was three years outside the events of 1789, to which the experience was believed by them otherwise to relate.¹

This is not the occasion to discuss the evidence for or against the popular belief. It must suffice here to record the relevant facts about this case, so that it can be assessed from that point of view. For this purpose the relevant date is 10 August 1774, or thereabouts. Can we judge what Richard's feelings were at that time? On 2 July Marie Antoinette was still busy examining plans,² including, no doubt, those of the Comte de Caraman and Richard. On 23 July the Queen, having formed a preference for M. de Caraman's plan, went to see his 'English' garden: 'As a result of that visit she definitely decided to adopt his plan, and confirmed his appointment as 'Director of the Queen's Gardens'. The next day he went to the Trianon to arrange matters.³ On 1 August the Court went to Compiègne for the summer holiday, and the House was presumably shut up. We can guess something of Richard's thoughts. Already his botanical garden was in process of being moved elsewhere. In the wilderness of the English garden his special knowledge and skill would be largely wasted. His very employment at the Petit Trianon was in danger, as the Bellevilles, gardeners at the Grand Trianon, were hoping to get his garden included in their province. Fortunately for Richard, he had a friend at Court, the Duc de Noailles, who saved his appointment.⁴ But it meant a complete submission to fate, as one can judge from the text of a declaration (undated) Richard made about this time, of which the following is a translation :

I only presented my plan to the Queen because Her Majesty ordered and requested it ; but as the Queen has seen fit to adopt another and to sign it with her hand, I will be subject to all orders from Count de Caraman and M. Mique, and I will carry them out faithfully, exactly and promptly.

[Signed] RICHARD fils⁵

It is certainly very curious that on 10 August 1901 two English ladies should have seen the garden not only furnished much as Richard had imagined it, but also deserted, save for garden and

¹ A, p. 57.

² Desjardins, p. 61.

³ De Nolhac, pp. 82-3.

⁴ Desjardins, p. 68. ⁵ A. N. O¹ 1883. Also de Nolhac, loc. cit., p. 87.

domestic staff and the enigmatic figure at the kiosk. The house, as they saw it, was shuttered, as it doubtless was on 10 August 1774, the Court having gone to Compiègne. The ladies also felt, for no assignable reason, depression and loneliness, emotions which must have been still acutely felt by Richard on 10 August, but which were quite inappropriate to their own private circumstances. Even their feelings near the kiosk, at the prospect of walking past the man marked by smallpox, were more appropriate to Richard's memories of Louis XV on his death-bed than to a casual encounter with a stranger whose face bore traces of the disease. They afterwards thought the figure was Vaudreuil, who was dark and had had smallpox. Their feeling of revulsion was not shared by the ladies of his day. Stryienski, writing about Madame de Polignac, remarks, 'The fair Comtesses was very susceptible to the handsome face and agreeable manners of Vaudreuil.'¹

If, in the light of the facts I have cited above, one is inclined to attach significance to the date of the experience in 1901, one must at the same time remember that no such significance can be attached to the dates of Miss Jourdain's two subsequent experiences (2 January 1902 and 12 September 1908).

5. MISS JOURDAIN'S THESIS

The question naturally arises whether there was anything in the mental equipment of either observer which predisposed her to an experience of the kind here at issue. My own impression is that one should first consider the case of Miss Jourdain, whose personality seems to have been more deeply involved from the start than that of Miss Moberly.

On 10 August 1901, Miss Jourdain, who was a stranger to the place, took the lead in going up the lane, as though she knew the way, and surprised Miss Moberly by not asking the woman at the window whether one could get into the Garden by that route. I have already given reasons for thinking that Miss Jourdain's trance was more continuous and more prolonged than that of her friend (pp. 134 and 136). It is unlikely that before this first visit Miss Jourdain had seen any of the sources on which I have drawn for support of the working hypothesis. From 1902 onwards she had a copy of Desjardins' *Petit Trianon*, which contains reduced facsimiles of two of Le Rouge's plates in Cahier 6, viz. No. 20 showing the lay-out in Louis XV's time (facing p. 18), and No. 19, Antoine Richard's *projet* (facing p. 64). It must also

¹ C. Stryienski, trs. H. N. Dickinson, *National History of France. The XVIIIth Century* (London, Heinemann), p. 236.

be assumed that in the course of her researches in Paris she saw Gabriel's plan. In short, if memory could work forwards, she could no doubt have furnished material from her own subsequent reading to externalize a vision such as she had in 1901.

It is unlikely that she ever saw Le Rouge's volumes. There is no mention of that source in the book of notes left in the Bodleian Library, and there is no copy of the work in that library. If Miss Jourdain had consulted the work in Paris, she would probably have noted the fact. I mention the above facts, in case they are of interest to those who believe that precognition is the key to the problem of apparent retrocognition. More pertinent, in my view, is the question whether Miss Jourdain's mind at the time was pre-occupied with any particular subject of study. Her special interest was the other-world symbolism of Dante. Between 1891 and 1894 she contributed papers on that subject to the *Expository Times*, and later worked these up into an Essay entitled *A Study in the Symbolism of the Divina Commedia* (Norland Press, South Devon, 1902). The dating of the brief preface, 'Paris, August, 1901', suggests that this subject was in her mind at the time of the experience. After this she elaborated the material into a Thesis in French, entitled *Le Symbolisme dans la Divine Comédie de Dante*, for which she was awarded a Doctorate of Letters in the University of Paris in 1904. In these essays I do not find any detailed clues to material in the visions, but it is, perhaps, noteworthy that this series of other-world experiences took place in a garden, which is a symbol of Paradise. In these eighteenth-century 'English' gardens, with their winding streams and grottoes, there was a conscious element of classical other-world symbolism, with which Miss Jourdain was very familiar from Homer and the *Aeneid*. I doubt, however, whether anything in her reading introduced the 'Chinese' element. But whether one regards the kiosk as having reference to an imaginary Chinese garden pavilion, or to the Jeu de Bague, with its mounts of dragons and peacocks, it is perhaps a matter for reflection that these are Chinese other-world symbols, a fact of which it is unlikely that either lady was aware.

Miss Jourdain in A (1st Edition, p. 100) claims to have been a person having powers of second sight deliberately undeveloped. This probably means that she had some previous experiences of a kind to justify the claim to second sight, but deliberately abstained from the exercise of it. This assumption is borne out by the fact that Miss Moberly, in her old age, used to tell a number of stories about the psychic experiences of her friend Miss Jourdain, which do not appear to have been recorded at the time of their occur-

rence. The lapse of time makes it impossible now to assess the value of such stories as evidence of the paranormal, but they may be of psychological interest, when read in relation to the case now before us. For that reason I recount here a brief story which has been communicated to me by Canon T. R. Milford, a relative of Miss Moberly, who told it to him nearly thirty years ago.

When Miss Jourdain was a young girl, she had several times the same dream. She was in a long eighteenth-century room or gallery, and at the far end there was a lady in the dress of the period, playing the piano (or possibly it was a harpsichord); always she was playing the same piece, and always before the end of it a servant came in at a door near to her, and said something, at which she got up and went out. Having heard the same piece several times, the little girl found that she could play it herself when she woke up, and her mother wrote it down. They thought it was beautiful and tried to find what it was, but no one knew it. Only afterwards they found it was a composition of Couperin, called 'Les Barricades Mystérieuses'. Neither of them, to their knowledge, had ever heard it before.

The dream, whether one considers it veridical or not, is interesting as showing an early tendency on Miss Jourdain's part to externalize an eighteenth-century background. François Couperin (1668-1733) composed 'Les Barricades Mystérieuses', and published the piece in Volume II of his *Four Books of Clavecin Ordres* in 1716. Mellers¹ describes it as 'a very famous piece'. The dreams presumably occurred about 1880, when the music of Couperin was much less accessible than it is today.

As regards Miss Moberly, we learn from Miss Olivier² that in her case also there were several stories of her second sight, less well known than her experience at the Petit Trianon. Miss Olivier recounts two of these, both visual hallucinations, of date about 1913. One of the visions was seen in Paris, at the Louvre, but in neither case does the subject matter appear to have any bearing on the series in A.

6. SUMMARY OF FINDINGS

In the following table I have shown on the left hand side a list of the nine main physical features which the observers saw as apparently spectral objects. In the next three columns, which relate to the garden plans of 1774 (Louis XV), 1774 (Richard's Plan), and 1783 (Mique's Plan), an *S* means that the object in the

¹ Wilfrid Mellers, *François Couperin and the French Classical Tradition*: (London, Dennis Dobson, 1950), p. 212. See also pp. 358 and 379.

² Edith Olivier, *Four Victorian Ladies of Wiltshire* (London, Faber, 1945), p. 40.

left-hand column is rated as significant of a corresponding object in the plan indicated. In the language of experiment, the entries in column 1 are 'guesses' which are being scored against alternative targets, to which they might possibly relate. Opinions will differ as to whether an *S* is deserved in every case where I have shown one. I think that in this case a 'near miss' can earn an *S*. If, as I walk through a garden, a bullet whistles past my head, that may be an accident. But if as I go from one point to another, the same thing happens again and again, I am justified in thinking that someone is trying to shoot me, even if he does not succeed in doing so. The target is not clear at the start, but becomes so as one walks along.

I have not included in the table a 1901 column, as it would be entirely, or almost entirely, blank.

If, as the result of studying this table, we regard Richard's Plan as being the ostensible target, it still remains necessary to consider whether, in their walk through 'Richard's Garden', the observers made any such startling omissions that their 'score' is a very low percentage of the features they ought to have noticed. This point is dealt with after the table.

TABLE

	1774 Louis XV	1774 A.R. Plan	1783
1. Isolated rock		<i>S</i>	
2. Kiosk (a) site of (b) appearance of		<i>S</i>	<i>S</i>
3. Bridge over small ravine			
4. Barrier on right		<i>S</i>	
5. Meadow with trees on left		<i>S</i>	
6. Grass up to terrace (north front)	<i>S</i>	<i>S</i>	
7. Door from second house, and level way to terrace	<i>S</i>	<i>S</i>	<i>S</i>
8. Road from French Garden near Chapel to Avenue	<i>S</i>	<i>S</i>	<i>S</i>
9. (2 Jan. 1902) Wood with winding paths	<i>S</i> (?)	<i>S</i>	<i>S</i> (?)

Notes

Item 2(b). *S* in 1783 Column assumes Jeu de Bague signified.

„ 3. Not identified with any certainty on any plan.

„ 8. Assumes that the road, shown as still there in 1783, was there in 1774.

„ 9. The winding paths are much more evident on Richard's Plan.

It will be seen from the above that, in relation to the walk of 10 August 1901, seven *S*'s were scored on Richard's Plan. Looking

at Fig. 3 (references are to Richard's numbers) one notices that no mention is made of the following objects which, one might suppose at first sight, were visible on the way from the gardeners' gate to the House; the small greenhouse (No. 4), the old orangerie (No. 2), the winding stream, the large greenhouse (No. 3), and the flower-beds inside the 'barrier'. Screens of trees or bushes are shown round Nos. 2 and 3, and No. 4 may well have been sheltered from the north by the lie of the land. It is quite possible that none of these three buildings was intended to be visible from the north. The ladies claimed to have crossed one stream by a bridge, but it seems to have been smaller than the winding river on Richard's plan, and it is hardly possible to identify the two.¹ Even so, the omissions are few and not such as to throw serious doubt on the working hypothesis.

The above appreciation does not take account of the possible significance of any of the persons seen, or of the appropriateness of the emotions felt. These, between them, might well be reckoned as earning three or four more *S*'s. I do not attempt to draw the results to a more exact total, as the force of the arguments cannot be fully expressed in terms of mere arithmetic. The evidence has proved to be 'convergent' along three different lines: first, in the visual field, looking at the series of features in Richard's plan, encountered in their right order; second, in the emotional field, in the light of the impact on Richard of the series of events in the three months after the death of Louis XV, and, third, the cumulative circumstantial evidence pointing to the influence of Sir William Chambers's designs on the imaginary 'kiosk'. This convergence of lines in different fields is very remarkable, and it is difficult to believe that it is due to chance. On the other hand, it is almost as difficult to believe that it is due to a causal factor of the kind I have suggested. Some people would say it was more difficult, not to say impossible.

It is the material which constitutes such an intractable problem. I do not think the general method of interpreting it which I have adopted, attributing a symbolic significance to some details, and a plain historical significance to others, is inadmissible. Such a combination of features is to be looked for in visions of this kind. In *Human Personality* (i, 276) Myers makes some observations on telaesthetic vision, that is, visions of scenes distant in space, which apply with equal force to visions of scenes distant in time. They

¹ The stream seems to have become smaller in retrospect. Miss Moberly's earliest description was as follows. 'In front of us was a slope leading down to a stream, which on our right hand fell over stones and was crossed by a rustic bridge.' 2nd. Edn. p. 185.

may appropriately be quoted here: 'And moreover through all telaesthetic vision some element of similar character is wont to run—some indication that *mind* has been at work upon the picture—that the scene has not been presented, so to say, in crude objectivity, but that there has been some *choice* as to the details discerned; and some *symbolism* in the way they are presented.'

Whether I have fairly applied the method I must leave the reader to judge. For my own part, I feel I must end with a declaration in terms reminiscent of those used by Antoine Richard:

I only wrote this paper because I found myself compelled to do so by the evidence. I will gladly submit myself to a more credible explanation, if anyone can furnish it.

I am grateful to M. Léon Rey for the benefit of his advice and information on several questions of historical detail about which I consulted him. For the manner in which I have made use of his assistance, I am, of course, entirely responsible. Mr W. G. Roll, President of the Oxford University Society for Psychical Research, has again very kindly made some researches for me at the Bodleian Library, and, in the preparation of Section 5, I have received valuable help from Lt.-Colonel H. F. N. Jourdain, C.M.G., and the Reverend Canon T. R. Milford.

HOME-TESTING ESP EXPERIMENTS : SPECIAL REPORT ON ONE SERIES OF TESTS

A CORRECTION

In Tables 2 and 3 of the above report (July–October issue of this *Journal*, pp. 159 and 160), the significance of the scores obtained was assessed by Stevens's method as well as by the Binomial method. It was subsequently suggested to the writer that only rarely does the Standard Deviation by Stevens's method exceed that by the Binomial method. On rechecking the calculations two arithmetical errors were discovered and the corrected results for Stevens's method are as follows:

Table	MCE	Dev.	SD	<i>t</i>	Old <i>t</i>
2	182.2	+85.8	11.99	7.16	5.80
3	80.3	+109.7	7.92	13.85	13.71

The effect of the errors was thus to under-estimate the significance of the results but the *t* values are in any case so high that small changes are scarcely meaningful. The conclusions drawn in the report are unaffected.

A. M. J. MITCHELL

REVIEWS

A NEW APPROACH TO PSYCHICAL RESEARCH. By Antony Flew.
London, Watts, 1953. vii, 161 pp. 10s. 6d.

In the introduction, Mr Flew describes the purpose of his book as 'frankly popular'. Psychical research 'enthusiasts' are warned that they 'would find nothing in the accounts of practical research with which they were not already familiar', and philosophers are warned that they 'would be horrified by the lack of professional subtlety and refinement in the more theoretical passages'. 'Laymen also', he adds, 'are entitled to consideration' (p. 1). This disarming introduction does not, however, absolve the reviewer from applying standards of criticism comparable with those which Flew employs when discussing what others have written. The novelty of approach which is claimed in the title consists, Flew tells us, in 'the combination of a resolute, yet not invincible, scepticism . . . with a constant awareness of language' (p. 2).

Chapter II provides a succinct account of what psychical research is, and is not. Flew then proceeds to classify the relevant phenomena, and he devotes a chapter to each of the following categories: spontaneous (mental), spontaneous (physical), mediumship (physical), and mediumship (mental). Chapters III to VI, which deal with these topics, will not be discussed here in detail, since the ground covered is so familiar to readers of this *Journal*. On the whole, these chapters are neatly and effectively written. Flew's main concern is to stress the difficulties and pitfalls for the investigator. Some will feel that this preoccupation has resulted in an unbalanced picture, particularly concerning the spontaneous mental phenomena. Flew describes only three such cases. His discussion draws heavily from Dr D. J. West's paper 'The Investigation of Spontaneous Cases',¹ and he quotes with approval West's conclusion: 'In the light of all the possible objections, and the obvious flaws in testimony of this kind, most of the cases seem worse than mediocre. The best known . . . seems dubious on close inspection' (p. 17). The only non-experimental phenomena which Flew takes seriously are those of mental mediumship. His survey of such phenomena covers much of the work done with Mrs Leonard, the 'Patience Worth' case, and the Cross-correspondences. Although Flew stresses, with good illustrations, the main pitfalls in this type of investigation, and the subjective element in any assessment of the evidence, he concludes that 'there is enough mediumistic material . . . of such a quality as to require us to postulate *some* paranormal factor' (p. 60).

¹ *Proc. S.P.R.*, Vol. XLVIII.

Chapter VII is entitled 'The Question of Survival'. In the first half, Flew discusses the question whether the phenomena of mental mediumship should be interpreted in terms of disembodied spirits, or of ESP involving only living people. Flew argues on familiar lines in favour of the latter alternative. He claims that these rival views 'certainly cannot be awarded the status of explanations', but only that of 'interpretations' (pp. 68-9). This claim does not, however, appear to be warranted, whether we use 'explanation' in an ordinary or in a scientific sense. Flew argues that 'telepathy is no more an explanation of the paranormal element in séance performances than memory is an explanation of our capacity to give our names and addresses'; but, whereas it is a mere tautology to ascribe the latter capacity to memory, this is certainly not true of the ascription of séance phenomena to telepathy between living people. The latter involves subsuming phenomena under a class to which they do not *prima facie* belong—a type of explanation common in the sciences. Flew goes on to argue that 'until or unless the concept "spirit" is made a great deal more specific . . . the spirit account cannot serve as a scientific hypothesis. To use it as such we should have to be able to deduce from it definite and testable consequences' (p. 70); and, he adds, we cannot do this, because 'with the spirits anything goes' (p. 71). This seems inconsistent with his recommending, as a method which 'promises big returns', the type of test designed by Dr R. H. Thouless, in which the author of an encyphered message has to supply the key after he is dead (p. 73). Flew seems here to be agreeing that this method does involve a testable consequence of the spirit hypothesis, which would *pro tanto* be a scientific hypothesis.

Flew passes, in the second half of this chapter, to 'a deeper level of criticism', the gist of which is that 'it is not clear what such a theory will *mean*' (p. 75). The issues raised here will already be familiar to members of this Society. Flew starts by repeating certain arguments,¹ which Professor H. H. Price criticised in his lecture 'Survival and the Idea of "Another World"',² and he goes on to comment on Price's criticism on the lines of his own recent letter in this *Journal*.³ A detailed analysis of this passage is therefore unnecessary, but the following comments may be worth making:

(i) Some of Flew's arguments in the preceding pages seem to require modification (or omission) in the light of his concession,

¹ These arguments appeared earlier in *University*, Vol. II, No. 2.

² *Proc. S.P.R.*, Vol. L, Part 182, pp. 9-11.

³ Vol. XXXVII, No. 674. (Price replied to this letter in Vol. XXXVII, No. 676.)

on page 80, that Price has indicated 'a conceivable mode of existence of possible conscious but incorporeal beings'. Flew does not, however, defend here his earlier conclusions that 'the tricks cannot be done', and that 'when we are dead nothing is experienced' (*University*, op. cit., p. 36).

(ii) Flew's main concern is to emphasise that the problem of survival involves 'decision issues', notably decisions concerning the use of the phrase 'the same person as'. Price, prompted by Flew's dictum 'people are what you meet', had written 'surely the important question is what constitutes my personal identity *for myself*'. Flew takes up this challenge, and argues that 'one cannot get around these points about *decision* issues by, as it were, appealing to a possible incorporeal being "himself" (or itself) to settle expertly whether or not "he" (or it) is Myers. (Unless, of course, one is prepared to abdicate the decision itself to the "spirits": which is itself a *decision*; and a very poor one)' (p. 82). The reason given for this conclusion is that 'in questions about personal identity even the honest testimony of that person does not necessarily provide the last word', since people's memories are fallible, and 'it is always possible to be *mistaken* as to whether one did or suffered something'. This is a surprising argument. Surely it is not necessary to insist that the memories of a (hypothetical) surviving spirit must be *infallible* before it could qualify to be called 'the same person' as a certain deceased mortal! Surely it would suffice if the memories of such a being were approximately as reliable, over a limited range of the past, as are the memories of us mortals. Presumably Flew is mistaken in some of his present memory-beliefs, but surely his conviction (knowledge?) of his own identity is based on what he does, or can, now *recollect*, supplemented perhaps by what other people now claim to *recollect*. If, after Flew's body) has grown cold, there occur experiences involving memories of many incidents in the life of (the embodied) Flew, surely the subject of such experiences would have good reason to decide that he (it) was in *one* sense of the term 'the same person' as (the erstwhile embodied) Flew. In view of this, why should Flew say that it is 'a very poor decision', to leave it to 'a possible incorporeal being' to decide whether he (it) is Myers? No one else (except an omniscient deity) could be in as good a position to make this decision.

(iii) Flew's arguments could have been made much clearer if he had distinguished two kinds of 'decision issues': (a) concerning the meaning to be given to words, e.g. to the phrase 'the same person', and (b) concerning non-linguistic facts, e.g. whether events which someone seems to recollect really happened. Such

decision-issues are not, of course, unconnected. Linguistic decisions are (or should be) made in the light of (what we believe to be) the relevant facts. From the viewpoint of psychical researchers who are 'in the flesh', the relevant facts are notoriously complicated and ambiguous; but what such facts *suggest* is, roughly, a continuation of *memories* and *desires* characteristic of deceased persons, and it is, presumably, by such criteria that psychical researchers would define *their* use of 'the same person'. Flew's contribution would have been more valuable if he had specifically analysed *this* usage. He does, however, make a formidable point when, in a footnote, he says 'suppose two incorporeal beings *both* claimed to be Myers, and *both* displayed the appropriate characteristics and "memories"' (p. 82). This ingenious example reminds us that the facts *might* turn out to be such as to confront us with embarrassing, and indeed arbitrary, linguistic decisions.

Chapter VIII ('The Experimental Study of Paranormal Phenomena') is rather disappointing. Since the best ESP experiments are the only findings of psychical research which, in Flew's view, decisively establish the reality of 'some unfamiliar factor', one would expect him to provide a more thorough survey of the evidence than in the previous chapters. His account of 'statistical significance' is not likely to help the layman to understand what is involved in evaluating experimental results. He states initially, as one of the advantages of the experimental method, that one 'can find out for certain whether the percentage of hits is significant' (p. 84). There is no hint in what follows that it is a matter of choice or convention what we take as our 'level of significance'. The nearest approach to a definition is the statement: 'The results with many subjects are *statistically significant*: more, that is to say, than can be discounted as what was only to be expected "by the law of averages"' (p. 86). He proceeds, however, to formulate separately three corollaries, the second of which involves that 'steady scoring of six or seven right out of twenty-five is very significant indeed, and the odds against its occurring by chance alone are monstrous', while the third involves that 'large numbers of guesses are essential if there are to be any results at all' (p. 88). No hint is given that in order to reach significance, at the chosen level, the rate of scoring and the number of trials are interdependent. In view of this, it seems somewhat incongruous that Flew should, at several points in this and the next chapter, present to the layman the highly sophisticated thesis of Mr G. Spencer Brown.¹ In a postscript to Chapter IX, Flew

¹ This thesis is discussed by the reviewer in a letter on pp. 292-4 of the present issue.—ED.

tells us that, after reading two of Spencer Brown's unpublished papers, he considers that the prospects of carrying through his 'Copernican revolution' are 'brighter than he had ever dared to hope'. It seems premature, however, to tell the layman, without elucidating the alleged connection, that 'the reporting of the PK correlations inevitably throws into question the accepted interpretation of the ESP ones' (p. 107).

Flew's survey of the ESP experiments is extremely brief—three and a half pages for the experiments with Shackleton, two pages for Carington's group-experiments with drawings, and less than a page for Tyrrell's work with Miss Johnson. Such experiments deserve a fuller and more careful account. Flew has not succeeded (probably no one could, in such brief compass) in giving a clear and accurate account of Carington's experimental procedure, method of assessment, and results. More space than is allotted to the experiments is given to 'cautionary tales' (e.g. Soal's exposure of 'Marion') and to 'drawing morals' (e.g. that work with star subjects is more promising than 'mass-experiments'). One of the points made here concerns repeatability—'that great scandal of ESP work: the fact that even the same subject in apparently identical conditions cannot be relied on to perform consistently well' (p. 100). In discussing this question it would have been appropriate to mention the extent to which the Shackleton phenomena *were* repeatable over a number of years. Also relevant, but not mentioned here, is the American work on psychological traits and attitudes. (Flew refers to this work only in a single sentence, on page 98). Dr Schmeidler's 'sheep-goat' experiment appears, for example, to have provided a repeatable group-experiment.

Flew confesses in advance to an 'almost invincible incredulity' regarding psychokinesis. He says of the American work that 'PK has only been detected statistically and often rather deviously at that (for instance, great weight is put on the evidence from the "decline curves"; showing regular declines in rates of success—the end of "*beginner's luck*")' (pp. 104–5; my italics). This parenthetical remark (the only reference in the book to the position-effects found in psi-phenomena) suggests a failure to appreciate the significance (in the ordinary as well as the statistical sense of the term) of the declines reported in the American work. The phrase 'beginner's luck' seems calculated to mislead the layman, since it will suggest that the declines in question are simply from earlier to later sections of the results obtained *by the same subject* (*beginner*); whereas the reported declines are primarily from earlier to later trials in the run and earlier to later runs in the set,

when the scores of the same *or* of different subjects are pooled. In the ensuing paragraph, where Flew lists the successful and unsuccessful PK experiments recently carried out in Britain, he ignores Mr Richmond's notable experiment with paramecia. Flew makes an important point when he insists that, to demonstrate the reality of PK, statistical evidence is not, in principle, necessary; that 'it is necessary only to show deflections in some instrument, correlated with "willings" by the subject, and under conditions which preclude the operation of any normal human or non-human agency' (pp. 106-7). This seems warranted; but, when he adds that the subject 'could have as many shots as he liked. *Even one success would prove the point*' (my italics), this seems too simple. A physicist who had personally conducted such an experiment *might* be convinced thereby, but would his colleagues, until or unless a recipe were found, enabling them to repeat it?

Chapter IX ('Describing and Explaining') opens with an attack on Dr Rhine's terminology, on the lines of Flew's talks in the B.B.C. Third Programme.¹ Rhine is accused of misconstruing the logic of 'mind-talk' and thus generating an atmosphere of mystification. But some of the things Flew says in this chapter are, in their own way, somewhat mystifying; for example:

(i) The model of wireless telegraphy is said to be inapplicable to telepathy, because 'all radiative effects have an intensity proportional to the distance from their source' (p. 114).

(ii) Flew recommends that if we must have a model in terms of which to think of experimental psi-gamma and 'to try to make it intelligible', we should use 'the model of guessing'. He adds that this is 'only offered as a convenient stopgap . . . which is not so grossly unsatisfactory as the popular alternatives. It does not even begin to provide an explanation of psi success' (p. 130). But is there any point *at all* in this recommendation? If it were adopted, it would be necessary to distinguish forthwith between normal and paranormal guessing, and the facts are not made any more intelligible by calling them 'paranormal guesswork'.

(iii) Most puzzling in its implications is the paragraph on pages 117-19. Flew defines 'psi-gamma' as 'the factor which gives rise to significant deviations from mean chance expectation *in a series of guesses*', and he tells us that it is only in this austere sense that he is 'prepared to concede that the ESP effect has been demonstrated'. He concludes that 'it would make no sense to speak of psi-gamma in connection with any *single* item of correspondence between a putatively paranormal dream . . . or what not, and what

¹ Reprinted in *The Listener*, 27 September and 4 October 1951.

had happened . . . it is not just that any single correspondence might *as a matter of fact* be a matter of chance, but that . . . we can *give no meaning to the question* whether it is or not' (his italics). If this represents Flew's deliberate conclusion, many passages in the book require to be rewritten. Consider, for example, the passage on page 60, where Flew argues that although spontaneous and mediumistic mental phenomena are not, taken by themselves, conclusive, we do not have to assess them separately, for there is also the experimental work; and that when this is taken into account 'we can be confident that some paranormal factor is often at work' in the non-experimental cases. Now the ESP experiments were prompted mainly by a desire to verify the view that the spontaneous and mediumistic cases cannot be written off as mere coincidences; and, on page 60, Flew implied that the experimental work *has* achieved this. Yet now he tells us that ESP has only been demonstrated in a sense which makes it *meaningless* to ask whether any single correspondence is an instance of ESP! What becomes, then, of the argument of Chapter VII, that séance phenomena should be attributed to ESP rather than to 'spirits'? The phenomena in question have almost always been of the type of which Flew now says that it is meaningless to ask whether they are instances of ESP! Anyone who wishes, like Flew, to adopt a purely operationalist definition of 'psi-gamma' or 'ESP' ought to ponder on the paradoxical implications of so doing.

This chapter does, however, contain some interesting and effective passages, for example, the account of the scientific postulates which seem to be violated by psi-phenomena (pp. 122-3). Flew concludes that since psi-gamma does occur, 'we shall just have to revise our ideas about what sort of place the universe is' (p. 124). He stresses that there is no need to abandon all our scientific principles, but in the ensuing argument he conveys the impression that the necessary revisions are, in principle, quite simple. He writes:

. . . apart from the anomalous set of very weak effects constituted by these excessively rare and elusive correlations, everything else is just as it was before. Once the correlations are admitted as exceptions to the various general principles against which they offend—until or unless either they become very much more common, or parallel phenomena are found in other fields—there seems no reason why most sciences (scientists) should be upset further. Of course $\psi\gamma$ is an untidy anomaly. Of course, one hopes that it will in time be fitted into some scheme of scientific theory and prediction. But even suppose it cannot be . . . this will mean only that . . . the universe is

not quite as we might have wished. Perhaps we tend to be spoilt by our constant success in formulating natural laws (p. 124).

And on the next page Flew tells us that we must not abandon, but simply reconstrue, the scientific postulates which are violated, e.g. the 'Postulate of Spatio-Temporal Continuity'. 'For this should be taken as an (invaluable) *heuristic maxim*, remaining sound in spite of our occasional failures (as here) to find what it bids us seek; not as a (mistaken) *fundamental presupposition*, now disproved.'

In this interesting passage, Flew *seems* to be arguing that psi-phenomena (in addition to having no metaphysical implications) have no far-reaching implications for science; that scientists need not *worry* about such facts, since they can be segregated and disregarded as a sphere in which Nature unfortunately provides no discoverable uniformities; that the only revisions called for are in our theory of scientific theory—in reconstruing certain postulates. It seems pertinent to point out that science as we know it would never have evolved, if Flew's policy had been followed by scientists when they were confronted by phenomena which violated current preconceptions; if, for example, nineteenth-century physicists had refused to be 'upset' by electrical and magnetic phenomena and had been content to say that the principles of Newtonian mechanics remained valid as 'heuristic maxims'.

The passage discussed above is not, however, Flew's last word. He does consider that we should go on trying to discover laws applicable to psi-phenomena and to construct theories. He suggests that perhaps the only applicable laws will be statistical ones; and although he has no new theory to offer, he gives a brief, but not unsympathetic, discussion of the theories advanced by Carington and by Thouless and Wiesner. Flew's concluding point, that 'a strong case can be made for saying that the research situation is not yet ripe for theory construction' (p. 132), seems justifiable. For scientific theories are normally constructed to explain sets of previously established generalisations about *classes* of things or measurable *properties*; whereas, in psychical research, we have, as yet, little that is definite to go on except generalisations about certain *individuals* (e.g. Shackleton)—and the generalisations applicable to one such individual are often not applicable to others.

In the last chapter, Flew gives, very briefly, his reasons for concluding that the findings of psychical research do not (i) support or undermine the Christian faith, (ii) have any ethical implications, or (iii) rule out epiphenomenalism (the theory 'that mental events are a by-product of neural events'). The reviewer would be

inclined to challenge the last of these conclusions, if he had not already done so elsewhere.¹ Suffice it to say that this problem is a difficult and controversial one. There are also two appendices, the first criticising the evidence of *An Adventure* on the lines of Mr W. H. Salter's Note in this *Journal*,² while the second criticises Mr J. W. Dunne's theory of time. Though the references at the end of the latter appendix include some marginally relevant papers, no mention is made of the *locus classicus*—Professor C. D. Broad's paper in *Philosophy* (Vol. X, 1935).

What, then, are we to say about Flew's 'new approach'? His policy of 'resolute, yet not invincible, scepticism' and his propensity to minimise the importance of psychical research are not particularly new; and it is not obvious that his 'awareness of language' has resulted in any important new insights, for when Flew does make novel suggestions, these do not seem to be the fruit of *linguistic* analysis. The book was written, we are told, for the layman, and there is a need for an up-to-date introduction written in a cautious and analytical spirit. Flew's book does not, however, seem ideal for this purpose. It is too brief and sketchy in its surveys of the evidence, and the layman (and others too) may often find the arguments in Chapters VII to IX difficult to follow; for Flew is here making sophisticated, but very condensed, contributions to the most vexed and difficult problems of psychical research. The second half of the book will be best appreciated by serious students of the subject, all of whom ought to read it. Serious students may regret that Flew has not given more space and time to working out his ideas, but everyone will agree that this book is eminently readable. It is written with the vitality, wit, and economy of style that we have come to expect of its author.

C. W. K. MUNDLE

TOUT L'OCCULTISME DÉVOILÉ. By Robert Tocquet. Paris, Amiot-Dumont, 1952. 327 pp. Illus.

This does not live up to the title. Chapters on supposedly genuine mediums alternate with chapters on pure fakery, and it is not always easy to appreciate the contrast. The photographic illustrations of the pseudo-phantoms of Stanislaw P. and Eva C. look much the same as those of Franek Kluski who, together with Jean Guzik, D. D. Home, Eusapia Palladino, and Rudi Schneider, is ranked among authentic materializing mediums. From the ancient style of the photographs, and the dates of most of the

¹ *Journal of Parapsychology*, Vol. XVI, No. 4, pp. 266–8.

² *S.P.R. Journal*, Vol. XXXV, No. 656.

phenomena, one might think the book was written long ago, but this is due to the fact that sensational mediums are practically extinct so that a book about them must necessarily appear rather historical.

It is perhaps unfair to complain of the inadequacies in the account of psychical phenomena since the main purpose of the book was to explain methods of fraud. This it does conscientiously, covering a wide range of depressing topics including stage telepathy by subtle codes and hidden transmitters, mediumistic impressions by physiognomy and conjecture, spirit photography by double exposure, materialization by cheese cloth, ghostly lights by phosphorus, and all the other hoary deceptions. The wonder of it is that so many generations of ghost-hunters have let themselves be led up the same old garden path. Clearly the operative factor is not the tricks themselves—which are always so painfully obvious when they are revealed—but the way they are put over. If you or I were to wave butter muslin or strike spirit lights, nobody would take us seriously. The guile of the successful faker creates the atmosphere of mystery in which such things may be accepted. M. Tocquet rather neglects this aspect.

Of some historical interest is the account of how the great Charcot was deceived by the sensation-loving women who so obligingly simulated trance and catalepsy for his classic demonstrations of hypnotism at the Salpêtrière. More startling is the chapter on physiological acrobatics describing the feats of Yogi who train themselves so to control their sphincters and intestinal movements that they can suck up fluids into the bladder and rectum. The same chapter describes the performances of the late A. G. Henskes (Mirin Dajo) who, believing himself invulnerable, allowed his chest and abdomen to be repeatedly transfixed by a narrow rapier. Unfortunately, the author rushes on from one fascinating topic to the next without stopping to develop any one in sufficient detail. To me at least it would have been more interesting to read some explanation of the physiological acrobatics instead of passing on quickly to a chapter of stage illusions. I wonder whether it was really necessary to include line drawings to show the exact position of the glass mirror required to produce the image of a disappearing lady.

D. J. WEST

THE STUFF OF DREAMS. By Alison Uttley. London, Faber, 1953. 255 pp. 12s. 6d.

This annotated collection was not made especially to serve the ends of psychical research, and though it contains a number of

precognitive dreams, they form only one section of a vivid and varied series. If they are read with the same ordinary instinctive confidence in the author's good faith as will automatically be accorded to those in the rest of the book, they will be found extremely interesting; though one or two may be thought to be telepathic rather than precognitive. Those not concerned to deny *a priori* the possibility of precognition may be inclined to speculate as to whether some of the dreams recorded in other categories do not also show traces of it; notably that of the Poison Leaves, which, allowing for the curious punning capacities of the subconscious mind, seems to hint forebodings of the use of Napalm in the Korean war.

The dreams are all dated, with the exception of one or two experienced during the author's days as a physics student at Manchester; she notes that she began to write them down after reading *An Experiment with Time*, though the majority are not connected with the future. All show the beauty, the vividness of apprehension, the sense of heightened significance to be found in her ordinary creative work.

The volume will be of no value to the statistician, or to the research worker who requires evidence of a legal standard before he is prepared to accept any incident as factual.

RENÉE HAYNES

THE LIVING BRAIN. By W. Grey Walter. London, Duckworth, 1953. xi, 216 pp.~ Illus. 15s.

This book, by the leading pioneer of electro-encephalography, is more than a popular description of the recording of brain waves. It is a succinct, ingenious, and witty account of the far-reaching implications of modern brain physiology. In the past decade the classical picture of the brain as an elaborate telephone exchange has been quietly superseded by a more dynamic conception. Now we have to imagine a vast self-regulating complex of feedback circuits which ceaselessly scan incoming impulses for significant patterns. At last some physical correlates of learning, memory, and even personality are beginning to be discovered. Brain rhythms are best known as aids to the diagnosis of epilepsy and cerebral tumour, but they are of much more general interest in that they provide a means of personality classification that is possibly more fundamental, and is certainly more interesting and more consistent, than most of the abstract psychological typologies. Encephalographic peculiarities are the first physical measurements that have been found to bear any valid relation to criminality.

It is in keeping with the wide sweep of Dr Grey Walter's approach that he takes the trouble to explain how all the available evidence is against the supposition that brain radiations can be the basis of telepathy. But the real interest of this book for psychical researchers is in the new context in which all future discussion of the mind-body problem will have to be considered.

D. J. WEST

SEARCHLIGHT ON PSYCHICAL RESEARCH. By Joseph F. Rinn. London, Rider, 1954. 396 pp. Illus. 25s.

Be reassured, gentle reader. This is NOT another book by the late Joseph Rinn. It is merely the English edition of *Sixty Years of Psychical Research: Houdini and I among the Spiritualists*, published in New York in 1950 and reviewed by Mr W. H. Salter in the May-June 1951 issue of this Journal. In this edition, which is mercifully shorter than the American, many of the grosser errors noted by Mr Salter have been omitted or corrected, but it would still be difficult to find its equal in this field for one-sidedness, smugness, and general ignorance of what psychical research really is. There are still traces of slipshod preparation. The photograph of Stanislava P., for instance, taken from Fig. 178 of Schrenck Notzing's *Phenomena of Materialisation*, is still captioned 'Madame Eva C. producing ectoplasm through a veil'.

O. B.

JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH. New York, A.S.P.R. \$1.50.

VOL. 47, No. 4, OCTOBER 1953

This issue is largely taken up with an important paper 'The Exploration of ESP and Human Personality' by J. Fraser Nicol and Betty M. Humphrey. It will be reviewed in detail in a later issue of the S.P.R. Journal. The paper was first presented at the first International Conference of Parapsychological Studies held at the University of Utrecht in August 1953. A short account of this conference is given by Dr Gardner Murphy. Professor C. J. Ducasse reviews *Tout l'Occultisme Dévoilé* by Robert Tocquet.

VOL. 48, No. 1, JANUARY 1954

Professor C. J. Ducasse discusses a number of the common criticisms raised by those sceptical of the reality of psychic phenomena, e.g. that they are impossible, or enormously improbable, that they are non-repeatable, explicable by fraud or collective hallucination, etc. He pays a tribute to the (British) Society for

Psychical Research for its integrity and pioneer work. It is surprising to find him paying an even warmer tribute to the late Harry Price. 'When testimony such as his in, for example, some of his experiments with Rudi Schneider is lightly dismissed, then questions relating to the psychology of belief arise concerning the very persons who raise them to justify their dismissal of that testimony.' Possibly so, but serious critics have never discounted Price *lightly*. Close examination of many of Price's reports reveals that they are peppered with errors and inconsistencies.

Frederick Knowles, some of whose early experiments in the relief of pain by mental concentration were reported in the *Journal* of the S.P.R. (xxiii, 1946, 194-200), here presents a brief review of all his work to date. He has made many laboratory attempts at relieving artificial pain, exerting PK force, influencing growth of seeds, etc., by the technique of mental concentration, but all were abortive. Nevertheless, he is convinced that he is able to bring about relief of pain in diseased persons, and he finds that scepticism on the part of the patient is not detrimental. I can testify to the difficulties of work in this field, as I was a guinea-pig in one of the early test sessions. I sat stripped to the waist while Knowles explored my skin with the point of a pin, intending to demarcate two reliably pain-sensitive areas, one for treatment and one as 'control'. No such areas could be found! The mere suggestion of the experiment had produced a partial anaesthesia.

Knowles studied orthodox medicine for several years to fit himself for this kind of investigation, and his paper is worthy of careful study. It is to be hoped that he will later publish a much longer report giving full experimental details.

Jan Ehrenwald reviews C. G. Jung's *Synchronicity: the Principle of Acausal Connections*; C. J. Ducasse reviews Margaret Murray's *The God of the Witches*, and Arthur Goadby reviews W. O. Stevens's *Psychics and Common Sense*.

DENYS PARSONS

CORRESPONDENCE

PROBABILITY AND PARAPSYCHOLOGY

SIR,—In his letter in the November-December 1953 issue of the *Journal*, Mr Spencer Brown has not accepted my invitation (in the July-October issue) to explain his theory by applying it to the Shackleton phenomena. May we assume that he now recognizes that this cannot be done? The only point in my letter

which Spencer Brown takes up concerns 'cross-checks': 'Dr Soal's first significant results with Mr Shackleton were obtained by means of what would be called, on Mr Mundle's own definition, a series of sixteen cross-checks.' This is not correct. I did not define 'cross-check', but I made it clear that I was using this term in its usual sense, i.e. the matching of a *run* of guesses against a *run* of targets for which the former was not intended.

Spencer Brown is naturally preoccupied with cross-checks since his theory implies that the extra-chance deviations which are found in ESP and PK experiments ought to be found *with equal frequency and of equal magnitude* when we match *any* two sets of comparable data. On his theory, the fact that the subject 'aimed' his guesses at certain cards, or 'aimed' his volitions at certain dice, has no relevance whatsoever in explaining the results obtained in such experiments. Spencer Brown refers us to only two cases which might appear to support his theory because so-called 'control series' have yielded significant results. These occur in Mr N. Richmond's experiment with paramecia and Mr H. Forwald's experiments with dice. (Spencer Brown mistakenly puts Coover's experiment in the same category, in his second letter in *Nature*.) It is therefore pertinent to point out that in *both* of these cases (i) the results in the 'control series' are *not* comparable in magnitude with those in the main series, and (ii) it is very doubtful whether the term 'control series' is applicable to the experiments in question.

In Richmond's experiment, the positive deviation in the main series gives a critical ratio of 9.28 ($P < 10^{-19}$), while the 'control series' yields a CR of 2.94 ($P = .0033$); and, in the latter series, Richmond did not eliminate the possibility of a psi-effect, since he selected a 'target' for each trial of the so-called 'control series'. Spencer Brown also directs our attention to the third section of Forwald's experiments in 'placement PK' (*Journal of Parapsychology*, December 1952). In the main series Forwald simultaneously released six dice, three of one material and three of another; and he tried to influence the three dice of a given material so as to make them come to rest in a certain area of the table (the table was divided into two equal areas—the 'A side' and the 'B side'). The 'influenced' dice showed a positive deviation, in 4,500 trials, yielding a CR of 4.0 ($P = .00007$), while the 'not-influenced' dice showed a negligible negative deviation (CR = .3). The only respect in which the behaviour of the 'not-influenced' dice was statistically significant was in respect of an 'order-effect' (i.e. the difference in the scoring rate between sets which started with the A-side as target, and sets which started with

the B-side). Presumably this is the 'cross-check' which Spencer Brown has in mind, but in magnitude this effect was of marginal significance ($CR = 2.62$, $P = .0088$), and Forwald did not treat the behaviour of the 'not-influenced' dice in the main experiment as a control series. (And surely he was right in this, since the 'influenced' and 'not-influenced' dice must have interacted mechanically during each trial.) Forwald carried out a parallel series of control experiments in which no attempt was made to influence *any* of the six dice thrown in each trial; and these series, he tells us, yielded chance results both in respect of total deviation and order-effect.

To sum up: although Soal's work seems decisive in eliminating Spencer Brown's theory as a possible explanation of ESP phenomena, and although Spencer Brown has certainly not substantiated his claim that in psychical research control experiments *frequently* give statistically significant results (*Nature*, 26 September), nevertheless, Spencer Brown has drawn our attention to a weakness in the evidence for PK, namely (i) the paucity of carefully designed control experiments, and (ii) the fact that, in two instances, (moderately) significant results have been found in what are (in his own wide use of the term) 'cross-checks'. Since our evidence for PK still consists very largely in the dice-work done at Duke University, I want to suggest a comparatively simple way of testing Spencer Brown's view that 'the claim of psychokinesis' is 'on the evidence, a nonsensical claim' (*Nature*, 25 July). Let us re-examine the Duke records of successful PK experiments, carrying out what Spencer Brown would call cross-checks. We could, for example, take as our unit the complete series of dice-falls which occurred while an agent was 'willing' each of the six faces of the dice in an equal number of trials, and we could match these against the differently arranged order of the six targets which was used in another similar experiment. (If Spencer Brown would prefer a different type of cross-check, let him state his preference.) If such cross-checks yielded deviations *and position-effects* comparable with those found by normal methods of scoring, this would effectively undermine the view that PK experiments have established a new kind of paranormal phenomenon; whereas, if such cross-checks consistently yielded chance-results, Spencer Brown's views about PK would no longer be tenable.

C. W. K. MUNDLE

Department of Philosophy,
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SIR,—After sending off my letter which appeared in your last issue, I noticed with regret that I forgot to mention that the paragraphs dealing with repeatability and Richmond's work were based on a paper by Drs R. Harrop and G. D. Wassermann which has been submitted for publication, and which the authors sent me. I should like to acknowledge the source of these statements.

S. G. SOAL

London, S.W. 12.

'BODIES' AND 'MINDS'

SIR,—In No. 664 of the *Journal* (May-June 1951) you published a letter in which I called attention to the dubious metaphysical assumptions betrayed by the use of language about 'bodies' and 'minds'. As the point is of vital relevance to attempts to integrate the findings of psychical research into the body of accepted knowledge, may I call attention to its bearing on certain later discussions?

The first is Dr Smythies's article in No. 666 (September-October 1951). On page 480 he quotes, with apparent approval, a criticism by E. L. Hutton of 'both realists and idealists' for their alleged 'confusion of the perceived body with the percipient's actual body'. Smythies holds that when we are considering a person's knowledge of the world we must postulate two worlds—the world-in-itself and the perceived world, and that a man's perception of his perceived world is something occurring in his 'mind'. The objects which a man observes and studies 'have their existence only in him' and the space-time system in which events are observed is the space-time system of his own mind (p. 481).

The truth is that, far from being 'confused', many philosophers have long been aware of this two-world theory, have understood it completely, and have quite consciously rejected it. The idea that there is a second world, a 'world-in-itself', lurking in the background and having a one-one relation to my perceived world, is quite uncalled for. All facts can be recorded quite adequately in language about people and their *perceived* worlds and the various mathematical and picture-thinking devices which they use to enable them to correlate their perceived worlds. It is true that most of us postulate that there must be some Cause or Ground for our perceived world, but we shall in the last resort have to designate this 'The Unknowable' or 'The Absolute' or 'God', so what is there to gain by interposing a dead world-in-itself? If we conceive the basic stuff, so to speak, of the Universe as sentient

beings, each with his perceived or experienced world, we can state in language appropriate to this conception all facts whatsoever about the Universe. Sentences about the causal or other relations between 'minds' and 'the' material world or between a person's body and his 'mind' can be translated into sentences about the relations, causal or otherwise, between changes in one person's perceived world and changes in another person's. We thus achieve an enormous simplification, since we have only one type of entity to deal with. A person's physical body is simply the sensory symbols in other people's (and his own) field of consciousness by which his existence and mental states are signalled to theirs.

It is clear from this that there is no need to conceive of a total space of higher dimensions in which perceived worlds and 'the real world' exist as parts.

My second example is Professor Price's important lecture 'Survival and the Idea of "Another World"'. He assumes that there is one common 'material world', that before death one is 'embodied', and that after death one's only possible existence would be 'disembodied' or 'discarnate'. At first one gets the impression that life after death is of a ghostly Hades-Sheol kind, but as one reads on hope begins to dawn. He rightly insists that any vividly imagined world is 'real' so long as it persists, and there is, of course, no logical contradiction in supposing it to persist indefinitely. But he also points out the possibility of telepathic apparitions—of communications from one person to another. If persons can deal, so to speak, in images and their emotional content, and also have the capacity for communicating with others at will by telepathically influencing their imagery, the future life may be conceived as a *society*. Life in such a society may be just as 'real' as earthly life; indeed, it may be far richer, for one of the limitations of our present life is our incapacity for expressing to others our deeper and subtler feelings.

But if we conceive the Universe in the idealist way I have outlined, the sharp contrast between our present existence as 'embodied' and our future as 'disembodied' vanishes. If the basic reality is the person, and the 'body' only a set of signals of such personal existence, then death can be regarded as merely the cessation of the use of one kind of signal; the person may well be able to use a new kind. In that case, communication between persons may be possible not only by telepathic apparitions but by sensory data—data as inconceivable by us as are visual sensations by persons born blind.

These considerations certainly ought not to be ignored in the discussion of the logical possibility, or intelligibility, of the idea

of survival. The answer to Professor Price's question, 'Could it be that these idealist metaphysicians have given us a substantially correct picture of the next world, though a mistaken picture of this one?' is that the idealist picture of *this* world cannot be shown by any empirical test, or by philosophical analysis, to be false.

F. H. CLEOBURY

Gainsborough, Lincs.

ESP, SPACE, AND TIME

SIR,—I should like to comment on one point of Mr Perry's letter (*Journal* xxxvii, 256). He mentions that precognitive dreams fulfilled within a shorter period appear to outnumber those fulfilled within a longer one, and attributes this to forgetfulness of the dreams. This explanation has some validity; but only in spontaneous cases. In an experimental series, the dreams are recorded, and the record read over every day for an agreed period. Under these conditions there cannot be any forgetfulness of the dream. Yet the number of precognitions is still greatest on the day following the dream, and falls off rapidly thereafter, though not so rapidly as in a collection of spontaneous cases.

This statement is based on three independent experiments—one by Kooy and Kruisinga, in which 1,444 dreams by the latter were recorded (*Tijdschrift voor Parapsychologie*, Vols. vi to xii); one by myself, with 56 dream records (*T. voor P.*, Vol. xvii, 86–90); and one organized by Dunne, with 7 dreamers and 88 records (*An Experiment with Time*, 3rd and subsequent editions, Appendix iii). The results were in reasonably close agreement.

G. F. DALTON

Dublin.

NON-CAUSAL SYNCHRONICITY

SIR,—This is a belated comment on Professor Price's review of *Naturerklärung und Psyche* in the January 1953 issue of your *Journal*. Professor Price concludes that Dr Jung's theory might be empirically falsified if, for example, we should one day find a causal explanation for apparitions. I do not see how this could possibly falsify the theory except in so far as it would apply to the particular cases for which a causal explanation was found. These cases would then simply be 'transferred' to the causal world order, but there would still be left plenty of 'meaningful' phenomena for the theory to live on, the type of phenomena for which everybody would agree that a causal explanation was unthinkable and therefore beyond scientific study, as for instance the Flammarion case,

the scarabaeus story, etc. Here 'Providence' would be an explanation; but wouldn't that be a causal one, though a type which science for good reasons prefers to ignore?

A. SLOMANN

Copenhagen.

SUBJECTS WANTED FOR A NEW ESP EXPERIMENT

Mr G. W. Fisk is now organizing an ESP experiment which aims at attempting to correlate a percipient's successes or failures with the *mood* he happens to be in at the time he makes his guesses. Mr Fisk is displaying three Clock cards on the wall over his desk. The cards are changed, in a random order, every 24 hours. Percipients are asked to make three guesses every day over a period of two to three months and, in addition to recording their guesses on the score sheet provided, to indicate also, by means of a pre-arranged code, their prevailing mood. As it will be necessary to accumulate a large mass of data if any indications of correlations are to emerge, Mr Fisk will welcome as many participants as possible, whether they be members of the Society or not. The experiment will only require about five minutes of a subject's time every day, and anyone interested is asked to apply direct to Mr G. W. Fisk, 6 Ditton Grange Close, Ditton Hill, Surrey, who will gladly supply full particulars.

ANONYMOUS DONATION

THE Council gratefully acknowledge the gift of £100 received on 19 January from a donor who wishes to remain anonymous. The gift is anonymous in the full sense of the word, for it was sent direct to the Society with no information as to the donor's identity. The Council are glad to make acknowledgement here in accordance with the donor's request.